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Summary Talk at Chiral '99*

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A brief summary of talks relating to massless lattice fermions is presented. This summary is not a review and reading it certainly is no substitute to reading the various original contributions.

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I. Introduction

By a rough count this was the third in the Chrial'XX series of conferences started in Rome in 1992. I guess that a summary ought to first reorder points made by various speakers by topics and then try to abstract generally accepted conclusions and identify issues on which agreement is lacking. As far as the first step, the data was subjected to severe cuts: there were several very interesting talks outside the narrow topic of massless fermions on the lattice which I shall not mention. From the talks that do concern massless lattice fermions I shall pick only what I think I understood; this is a major cut. I apologize in advance for omissions and misunderstandings.

The coarsest classification of the topics is into two classes:

- Chiral gauge theories.
- Vector-like theories with global chiral symmetries.

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II. Chiral gauge theories

Let's walk through a list of issues of principle on which I shall present a status report and, at times, and my personal opinion in a different font.

- There exists no complete construction of asymptotically free chiral gauge theories where the symmetry that is gauged is perturbatively non-anomalous.
- There is a disparity in beliefs on whether we have passed the point of "physical plausibility". By this I mean that, as physicists, we have established so many features that the remainder of the problem can be "shipped over" to mathematical physics, where in due time (hopefully $< \infty$) all hairy technicalities will be nailed down. But, we no longer have serious doubts about the outcome. Most of us would agree, for example, that the RG framework is far beyond physical plausibility. Nevertheless there is no mathematical proof beyond perturbation theory that there always exists a hierarchy of fixed points ordered by degrees of stability with appropriate connecting flows, etc.

  My opinion is:

- *The older approaches [1, 2] still are below the point of "physical plausibility". On the other hand, the new approach is past the point of "physical plausibility". I think many of us here disagree on this assessment.*
- *There exits only one new approach [3]. It is obvious, even if not represented at this conference, that there are some workers worldwide that would disagree with this.*
- *I think that most criticisms of the new approach, e.g. [4], are rooted in the difficulty to make the new approach look completely conventional.*

II-1. Unconventional features of the new approach

The new approach is unconventional in that the chiral fermion determinant is (at the first step at least) not gauge invariant, but the fermion propagator is gauge covariant. This implies that the fermion determinant and the fermion propagator are not related in the conventional manner. In the continuum this issue also exists although it is hidden behind the overall formal character of the path integral formulation. Fujikawa, in his work on anomalies associated this feature with the fermion integration measure rather than with the determinant but this separation is artificial because we see only the product of the "measure" and the fermion determinant, at least to any order in perturbation theory.

Nevertheless Fujikawa's view consists of a deep insight, not as much in the terminology, but because it tells us precisely what I just mentioned above: the fermion propagators are well behaved under gauge transformations, only the fermion determinant is not so (in the anomalous case). In diagrams this means that anomalies only come from triangular fermion loop insertions, and when phrased in this way it sounds less surprising. But, on the lattice there is no such thing as an integration measure for fermions: There are no infinities and Grassmann integration has nothing to do with measures. So, on the lattice one must do something somewhat unconventional to get the fermion determinant break gauge invariance while the fermion propagator does not. In the continuum, when anomalies cancel, we can get rid of the gauge violation in the fermion determinant and we might expect a totally conventional formulation to hold. There are some conjectures how to ultimately achieve this on the lattice, but nobody has done it yet. I think that to actually achieve this in full detail will end up having been unnecessary.

- The new approach requires us to choose bases in subspaces of a finite (if the lattice is finite) dimensional vector space. This choice depends on the gauge background. The definition of the spaces is gauge covariant but the choice of bases is not.

  In my opinion

- *the ambiguity in phase choice that results from the above is best interpreted as a descendant from an ambiguity in an underlying path integral of conventional appearance but over an infinite number of fermion lattice fields. There is no doubt that this is a possible interpretation*†*, because the new construction has been obtained from a system containing an infinite number of fermions and integrating all but the lightest out. The effective theory governing the lightest fermion can be formulated directly and then the infinite number of fermions picture is no longer necessary in the framework of Euclidean field theory. But, if one wishes to give some argument for why the theory should be unitary after taking the continuum limit and subsequently analytically continuing to real time, the single known way to date is to go back to the infinite number of fermion language, where one has a familiar form of lattice unitarity, at least at a formal level.*

- It is at the stage of making the phase choice that the obstructive role of anomalies shows up. It is also at this stage that possibly new obstructions could come in, "non-perturbative anomalies" [7].

† Could the Heat Kernel approach of [5] provide another interpretation ?

I believe that

- *no such problems will occur in many "good" theories, but I won't exclude cases we would deem good today, but find out that they are bad tomorrow. Some complications in finite volume in two dimensions might contain a hint* in this direction.

It is important to emphasize that the fermions enter the action bilinearly. The bilinearity has significant consequences and the entire new approach is dependent on it. Bilinearity means that all one needs to know about the fermions is their propagator, the fermion determinant and the possible 't Hooft vertices, all functions of the gauge background. In trivial topology, there are no 't Hooft vertices to worry about, and bilinearity gives a simple prescription for the result of the integral over fermions for any set of fermionic observables. This is the content of Wick's theorem. The extension to nontrivial topology with the help of inserting 't Hooft vertices requires some extra functions (zero modes). If we have the propagators, the zero modes (when present) and the fermionic determinant we know all there is to know and whether we also employ and action and Grassmann integration is a manner of notation but not substance. What is unconventional for a lattice theory is that the fermion propagator does not fully define the fermion determinant. Just like in Euclidean continuum, it does define the absolute value of the determinant. The phase of the determinant however needs to be determined separately. The main conceptual obstacle overcome by the overlap construction was concretely realizing this apparently paradoxical situation.

II-2. Phase choice and fine tuning

- What is missing at the moment in the asymptotically free context is a full natural choice of the phase of the chiral determinant making it explicit that if anomalies cancel gauge invariance can be exactly preserved but, if they are not, such a choice cannot be made by locally changing some operators.

But, we have some partial results:

- If anomalies do not cancel one can show that a good definition of phase, at least within one framework, is impossible.

- In the case of $U(1)$, if anomalies do cancel, at least in a rather formal infinite lattice setting, one can find a good definition of the phase of the chiral determinant.

I believe that

- *the problem of finding a good phase is almost entirely a technical problem. I also believe that it is a hard technical problem, at least at finite volume.*

Let us now turn to the issue of fine tuning which generated much discussion. First of all even the concept of fine tuning isn't perfectly well defined. I'll adopt the following definition: Fine tuning is the need to choose some functions of field variables which, when viewed as a series in elementary functions of fields, contain numerical coefficients that have to be of some exact value, with no deviations admitted. The numerical values of the coefficients are not directly determined by a symmetry principle.

- The solution to the technical problem of phase choice, according to all conjectures and results to date, requires fine tuning somewhere.

I believe that

- *if a solution to the technical problem exists, that solution defines a neighborhood, a region in coupling space, so that for any point in it the correct continuum limit will emerge after gauge averaging. So, you only need to be in a good neighborhood, not exactly at its center. This, in my definition eliminates fine tuning, but we had some disagreements both on whether this can work and on whether if it does work it really is natural. The basic way this is pictured to work is that in the anomaly free case one can do a strong coupling type of expansion in the deviation from the ideal point in the center of the neighborhood. One cannot see this work in weak coupling perturbation theory.*

- Currently there is an effort to define the phase of the chiral determinant in a perfect way. Kikuakwa's work on the η-invariant [6], Lüscher's attempts in the non-abelian case, including their respective conjectures are all part of this effort. The conjectures I presented in my talk are an earlier, somewhat different attempt in the same direction. In my attempt I tried to restrict all fine tuning to gauge covariant operators, while in the newer way one fine tunes at the non-gauge covariant level.

In practice I think one shall need to rely on the existence of the "good neighborhood" and try to guess a phase choice residing in it. There is numerical evidence that the Brillouin-Wigner phase convention (maybe more appropriately termed the Pancharatnam convention), at least in two dimensions, provides a realistic possibility.

II-3. Future

- A successful conclusion of any approach to find a perfect phase choice would constitute a significant result in mathematical physics.

Some personal opinions:

- *I am not convinced that we need many people working on this. We should all be happy if this issue is taken out of the way by somebody. The likelihood that new physics would emerge from a full solution of this problem is not high.*

- *Technically, things might simplify if one starts by considering more closely a mathematical construction directly at infinite lattice volume.*

III. Vector-like gauge theories with massless fermions

In this area there was a substantial amount of progress recently and contributions have been both original and coming from many people. The activity here is closely connected to numerical QCD and therefore of potential importance to particle phenomenology.

- *I think in this area there are easier open problems. On the other hand there are no fundamental open issues even at the level of mathematical physics (like the phase choice in the chiral case). We can have confidence in the basic premise that we know now how to formulate QCD with exactly massless quarks on the lattice.*

III-1. Numerical QCD

We have heard about two basic implementations of the new way to make fermions massless.

- Domain Wall Fermions, (DWF), the more traditional approach, were reviewed by Christ [8].

- Overlap fermions, a bit newer, were discussed by Edwards, Liu and McNeile [9, 10, 11].

What are the advantages of these new methods, when compared to employing Wilson fermions, say ?

- Small quark masses are attainable without exceptional penalties and without having to go to staggered fermions with the associated flavor identification difficulties. But, the price is still high. Actually, with DWF we only saw something like $\frac{m_\pi}{m_\rho} \sim .5$ while we really would like $\frac{m_\pi}{m_\rho} \sim .25$. To go so low a prohibitively large number of slices in the extra dimension seems to be required [8]. On the other hand we heard a report of attaining $\frac{m_\pi}{m_\rho} \sim .2$ with overlap fermions [10].

- *My guess is that the overlap went to lower masses because of the so called projection technique which allows a numerically accurate representation of the sign function down to very small arguments. This could be done also with DWF, but would be costly, because the transfer matrix is more complicated than the Hermitian Wilson Dirac operator. It would be illuminating if DWF people were to test the projection method in their framework, only to potentially identify the badness of their implicit approximation to the sign function at the origin as a possible source of the problems they encounter when trying to go to lower quark masses.*

- Related to my comment above, we have seen also first steps in the design of an HMC dynamical simulations method for overlap fermions incorporating the projection technique [9].

- One has very clean lattice versions of topological effects and the related $U(1)_A$ problem. Both DWF and overlap work give very nice results. For example, we saw that indeed $U(1)_A$ is not restored at $T > T_c$ [8], that Random Matrix models work as expected also at non-zero topology [9] and that the condensate $\bar{\psi}\psi$ behaves as expected [8, 9, 10].

- It is potentially very advantageous to have a formulation where operator mixing is restricted just like in the continuum. This can provide substantial numerical progress on matrix elements. There are good previous results on the Kaon B-parameter and surprising new results on $\frac{\epsilon'}{\epsilon}$ [8].

- *A natural question is then what can be done with the overlap in this context. There is a big factor difference in the machine sizes that are applied to DWF versus overlap, so we may have to wait for quite a while.*

- A cloud on the horizon has been discussed extensively [9]. It has to do with the fact that the density of eigenvalues of the hermitian Wilson Dirac operator H_W at zero seems not to vanish on the lattice at any coupling. This might indicate a serious problem since the definition of the overlap Dirac operator involves the sign function of H_W. The problem also directly affects DWF, making absurdly large numbers of slices necessary. The overlap permits a simpler fix. But, the problem isn't serious so long one works at fixed physical volume. In that case, taking the scaling law shown by Edwards, [9], we immediately see that, in principle, going with the lattice β to infinity at fixed physical volume will eliminate the low lying states of H_W^2. How to avoid the problem at low values of β, say $5.85, 6.0, 6.2$, is an open and practically important question. Several options were discussed, including changing

the pure gauge action and changing the form of H_W. In this context there might be some relevance in the new exact bounds on the spectrum of H_W^2 which were not yet complete at the time of the conference. These bounds were derived using also eigenvalue flow equations. Such equations were emphasized by Kerler in his talk [12].

- The main advantage of DWF over overlap fermions is the lower cost in dynamical simulations. It seems possible to combine the good features of DWF with those of overlap fermions using various tricks mentioned by Edwards [9]. There are many possibilities and we should be imaginative.

III-2. Non-QCD

- Kaplan discussed DW formulations of SUSY theories with no matter. In the continuum, with $\mathcal{N} = 1$ supersymmetry, the masslessness of the gaugino is known to imply supersymmetry at the renormalized level.

- Going to higher $\mathcal{N}$ supersymmetries employing dimensional reduction might not work [13].

- The fermion pfaffian related to the lattice gluinos was shown to be non-negative, thus eliminating a potential thorny numerical problem [13].

- Lower dimensional theories might provide interesting playgrounds [13, 14]. In particular some simple 3 dimensional gauge theories with massless fermions might have interesting symmetry breaking patterns.

III-3. Ginsparg-Wilson Relation, Index

- The Ginsparg Wilson relation is an algebraic requirement best thought of in terms of Kato's pair [3]. We had some discussion about the GW-overlap equivalence and the role of the operator R in the GW relation, see [15].

- The following formula for the index is reminiscent of the continuum treatment of Fujikawa.

$$\text{Index} = Tr[sf(h^2)] \tag{1}$$

where,

$$h = \frac{1}{2}\left[\gamma_5 + \text{sign}(H_W)\right], \quad s = \frac{1}{2}\left[\gamma_5 - \text{sign}(H_W)\right], \quad h \equiv \gamma_5 D_o, \tag{2}$$

and $f(0) = 1$. There might be some connection between this and Fujikawa's talk here [16], which centered on the operator s (the formula $s = \gamma_5 - h = \gamma_5(1 - D_o)$ is slightly different because of different conventions involving factors of two).

- We saw an analytical calculation showing that the lattice reproduces the correct anomalies even in backgrounds which are non-trivial topologically [17]. Previously, this has been checked only numerically and in two dimensions.

III-4. Future

There clearly is more to do and we have some good prospects for progress. On the numerical front further investigations of ways to implement the overlap Dirac operator, or of some equivalent object, are called for. While DWF are easy to visualize, and indeed produce, in the limit of an infinite number of slices, the sign function of $\log T_W$ where T_W is a transfer matrix and $\log T_W$ is the same as H_W up to lattice corrections, I see a danger in the concentration of large amounts of computer power on this one version of the new way to put fermions on the lattice. Once too many cycles are invested in DWF, better ways will get suppressed for a long time and, if any of the hints we are already seeing develop into serious obstacles, there will be no developed alternatives. This would cause delays in translating the beautiful theoretical progress we are witnessing into better practical number acquisition. In short, I urge DWF implementers to be more broad minded; control over a large machine comes with a large responsibility.

IV. Conclusions

It is rare that a subfield of theoretical physics solves one of its longstanding problems in a direct and "honest" way, rather than redefining it. Such a rare event has taken place in the context of lattice fermions. The solution may have implications for physics beyond the SM, because it is a way to fully regulate a chiral gauge theory, outside perturbation theory. This lattice theoretical development holds promise also for SM phenomenology because it could change substantially the methods of numerical QCD.

At the moment there are some tensions in the field surrounding issues of priority and implementation. These problems would get solved if we had:

- More imagination.
- More young people.
- More computing power.

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References

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